Syria
Archéologie, art et histoire
Recensions | 2021

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Maurice Sartre

Electronic version
URL: https://journals.openedition.org/syria/11213
DOI: 10.4000/syria.11213
ISSN: 2076-8435

Publisher
IFPO - Institut français du Proche-Orient

Electronic reference
Maurice Sartre, "Paolo CIMADOMO, *The Southern Levant During the First Centuries of Roman Rule (64 BCE-135 CE). Interweaving Local Cultures*", *Syria* [Online], Book reviews, Online since 14 September 2021, connection on 16 September 2021. URL: http://journals.openedition.org/syria/11213 ; DOI: https://doi.org/10.4000/syria.11213

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REFERENCES

Paolo CIMADOMO, *The Southern Levant During the First Centuries of Roman Rule (64 BCE-135 CE). Interweaving Local Cultures*, Oxford-Philadelphia, Oxbow, 2019, 216 p., ISBN : 978-1-78925-236-5.

1 In this book, which is an edited version of Paolo Cimadomo's doctoral dissertation (University of Naples "Federico II", 2017), the author seeks to examine the Southern Levant during the two first centuries of Roman domination. The Southern Levant, which for most authors extends from the border between Lebanon and Israel, in the North, and to Gaza, in the South, is narrowed down by the author, without explanation, to a much smaller region. In this manner, this work actually deals with a well-defined geographical space (the Galilee), two peoples (the Ituraeans and the Nabataeans), and an administrative structure (the Decapolis). For the Nabataeans, the author considers only the northern part of their kingdom, and writes about this space especially in the chapter Decapolis (ch. 4). But when speaking of the Decapolis, he neglects populations who occupied the interstices, the village populations outside the cities. The author clearly misunderstands the geographical configurations of this region. This is the result of his conflation of a geographical term, two ethnonyms and an artificial district. The link between these concepts is essentially their geographical proximity and their common political destiny. Despite the absence of even the slightest summary of the politico-administrative history of the region, which I readily admit is incredibly

complex, the fact remains that all were in the power of Rome since 64 BCE and were eventually integrated into a single Roman province in, 106 CE for the latest, the kingdom of Nabataea. Today, the regions in question are simply called Galilee, Beqaa, Jawlan and Hauran and they represent only a very small portion of the "Southern Levant."

2 Though there is clearly sufficient material for a doctoral thesis, the definition of a unifying research question for this otherwise disparate set is essential and would have been judicious. The author, however, hardly seems to concern himself with this, and presents neither the current state of research, nor the sources that should guide his own line of questioning. Instead, he devotes a long theoretical first chapter to demonstrate that he is aware of and understands the fashionable vocabulary, but does this without any regard for its relationship to the region of study. In the age of globalization and networks, it is important to highlight how these regions participate in the prevailing globalization and interconnectivity of cultures, without forgetting to speak of ethnicity. What can only be referred to as pseudo-scholarly eye candy may be appealing, but it is no substitute for an exact knowledge of the facts and the previously existing literature. And in this regard, PC's manuscript is very far from the minimal requirements for a work warranting the conferral of a doctoral degree.

3 Readers familiar with these regions will be surprised by the number of obvious, and sometimes very astonishing errors, including, but not limited to: the failure to consider many documents essential to the analysis of this subject, or their biased use due to a shocking ignorance of ancient procedures and realities, as well as incomprehensible digressions (the fixed chronological framework seemingly forgotten most of the time), which unnecessarily expand the text. Essential concepts, especially geographical ones, are used in an erroneous way from cover to cover. For example, PC uses the modern "Hauran" to designate the term *Auranitis* (which the author seems unaware also encompasses northern Jordan), and which is found in particular in Josephus. But Josephus almost always names *Auranitis*, *Batanaea* and *Trachonitis* together. Which begs the question, why add the last two toponyms if the first is enough to designate the whole? That question deserves at least a discussion that PC neglects entirely. It is possible that *Aurana*, quoted in the Zeno papyri, refers to the wholebasaltic country, like "Hauran" today, but this is far from proven. In any case, in Josephus, *Auranitis* designates, in its administrative form, a Herodian district (and only the Herodian one) of contemporary Hauran, entrusted by Augustus to Herod the Great at the same time as the plain of *Batanaea* to the West and the basaltic plateau of *Trachon* in the center (modern Leja); *Auranitis* is only the mountain to the East (mod. Jebel Druze or Jebel al-ʿArab). Herod only administers its northern portion, and the region continues south into the Nabataean kingdom. Josephus uses precise administrative terms that PC does not explain and, probably, does not understand. These irksome instances of ignorance, lead PC to draw seriously erroneous conclusions, which should have been corrected before publication, as it is inconceivable that the thesis jury did not take notice of them. Thus, PC seems convinced that Augustus bequeathed to Herod territories that were taken from the Nabataeans (p. 67), which is false, as the Nabataeans were as much Rome's clients as Herod. This oversight may be the origin point of another error, which will make any connoisseur of the Hauran flinch: the *Dousaria Actia* of Bostra are said to have been founded in the 1st century by "Philip" (p. 73), but PC never indicates which Philip. At this date, only Herod Philip would have had the necessary authority to found

a festival. But what would a Herodian ruler be doing in Bostra, a major city in the North of the Nabataean kingdom? In fact, this agonistic festival was created in the middle of the 3rd century CE by the Roman emperor Philip the Arab (born in Arabia), and a careful reading of Z. Fiema (*ZDPV* 2003), to which the author makes reference, would have avoided this enormity! Clearly, failure to examine the terms one uses precisely and ignorance of the political complexity of the region result in such preposterous claims.

4 After this pseudo-methodological first chapter, three main chapters are devoted to the discussion of regions or peoples. First, the Galilee (a "melting pot of different peoples," p. 22-46) is considered. The numerous works that have been devoted to this country over the past half century, as well as archaeological discoveries, have greatly improved our understanding of this entity. PC catalogues them, in part and in a disjointed manner, but he mostly engages in multiple lengthy digressions of little use. Did we have to, for example, go back to the Assyrian period for the history of the settlement? What matters for the subject is whether or not, in 64 BCE, there were many Jews there, or if they were the majority. PC sheds no new light on the question and sticks to the observation imposed by the discoveries at Sepphoris, that of a Judaism open to the dominant Greco-Roman culture. Non-Jews were undoubtedly quite numerous, but I do not believe that this can be deduced—as PC writes—from the discussions of the rabbis of the 2nd-4th centuries. The fact that they often discussed the cohabitation between Jews and Gentiles does not prove that this was a major preoccupation of the Jews of Galilee, but rather stems from the rabbis' awareness that the Jews were now more numerous outside the Holy Land than within. The documentation demonstrates an important use of the Greek language and the adoption of Greek ways of life. The lack of Eastern Sigillata A, as well as of Phœnician semi-fine ware, however, militates in favor of a broad Jewish domination and, in this light, Galilee may be understood as closer to Judea, where these ceramics were also missing. Despite a necessary parallel with the neighboring Hauran being at the heart of the subject, it is omitted by PC. As in many passages of the book, PC does not seem to grasp the limitations or potential gains provided by an astute reading and analysis of the existing documentation.

5 Chapter 3 focuses on "the Arabs" ("The Arabs in Southern Levant," p. 47-87), in reality only the Ituraeans and the Nabataeans, although the kingdom as a whole will never be discussed. We do not know what PC means by "Arabs," except for a brief allusion to his definition at the very end of chapter 4. The definition is critical, however, as this notion has a precise meaning among the Greek (and Latin) authors using this term, for whom Arabs are understood to be: pastoralists, often transporting goods via camel. The term "nomads," for these authors, also implies what is understood as a natural inclination towards banditry. In the ancient literature, "Arabs" are never farmers in villages. There is no linguistic or cultural connotation in their definition of Arabs, although they were known to have gods and a strong tendency towards aniconism; but they are not the only ones. Specialists are well aware that these nomads, who wrote a lot throughout the desert, as well as in their few oasis cities (Petra, Hegra), never used this term to refer to themselves, except for small groups, in the later periods. Michael Macdonald's work has significantly contributed to moving the issue forward, but the fact remains that we cannot discuss "Arabs" without precisely defining who we are talking about. PC is certainly aware of the difficulties of this terminology, but his analysis of the Ituraean question does not improve our knowledge of the subject in the least: were the Ituraeans Arabs or not? As we do not have any text in their vernacular language, we cannot even

relate them linguistically. Should we distinguish them from the "Arabs" as Strabo does, or not? Following the very comprehensive article written by Julien Aliquot on the topic, it is certainly difficult to contribute something novel and decisive. There is, however, one specific issue about which I am in agreement with PC, though I believe anyone would be: presently, there is no reason to specifically attribute the common ceramic called Golan Ware to the Ituraeans. Even if it is found almost exclusively in areas where the Ituraeans were living, i.e. Beqaa, Jawlan, Trachon, here, we rather deduce the presence of Ituraeans from the presence of said ceramics rather than the reverse. They were, of course, not alone in these regions and a correlation between this production and a specific population cannot be established.

6 The comparison that PC details with the Nabataeans is all the more fictitious as the entire southern half of the kingdom, its historic heart, is left out. This time, PC specifies what he is going to talk about: the Nabataeans, an Arab population living "in and around the Decapolis" (p. 60). Has the Decapolis become a defined geographic space? I will return to this point, but at the outset, the reader is left without an understanding of who these Nabataeans are, besides the knowledge that they possessed a great kingdom until 106, but nevertheless lived outside of it. In fact, this "definition" is preposterous, as people inhabiting the countryside between the Damascene and the Transjordanian plateau were sedentary Aramaeans, mixed with Arabs from the Harra, the eastern desert edge, and perhaps with some other Arabs from the South and some nucleus of Judaeans, Greek, Idumaeans, and Babylonians colonists. The Semitic onomastics of the region (in Greek, Aramaic or Nabataean) underscore a very strong identity specific to the Hauran, but it extends equally well to the plateaus of Ammanitis and Moab. As it is identical in the North of Hauran (which was only rarely and briefly dominated politically by the Nabataeans) and in the South of the region (incorporated into the kingdom over the course of the 2nd century BCE), the Nabataean domination must not have anything to do with it. By ignoring the origins of the settlers, PC ignores all issues and considers everyone to be an Arab! This is a particularly frustrating attitude when one considers that ethnicity was announced as a major preoccupation in the work's introduction.

7 After ignoring this essential question, PC continues on a path littered with mistakes which give rise to worrisome questions: was the candidate supervised and did the jury read the thesis? Because no, the reinforcements sent by Malichos I to Caesar during the Alexandrian War were not intended to support him against Pompey, who would have already been dead for six months at this point in time (p. 66). No, the battle of Canatha in 31 was not a victory for Herod, and the districts entrusted to Herod a little later by Augustus were not taken from the Nabataean kingdom (p. 67). No, King Obodas did not die in 4 BCE (p. 68). And no, Aelius Gallus's expedition to Arabia Felix was not a failure (p. 67): Augustus already stated otherwise, and today's historians absolutely agree with him.

8 The Nabataean culture, which we know well, is more present in the South of the kingdom than in the North, where it was mainly a political presence throughout this period, at least until the annexation of 106 (why question the date?). PC tries to collect what he can over this fairly well-documented period, thanks to numerous Nabatean royal inscriptions (cf. Laïla Nehmé's inventory, 2010). However, the Nabataean "cultural" example that he develops is Sia, the great Aramaic sanctuary near Canatha. Peter Alpass, who is quoted by PC, but either not read or not understood, already

positioned himself against (in 2013 and with good reason) the attribution to the Nabataeans of a sanctuary located in an area which they only rarely and very briefly controlled (for a few months following the battle of "Canatha"). This reality, however, does not prevent PC from declaring without hesitation that the sanctuary is recognized as Nabataean (p. 80); this was true over a century ago, at the times of Vogüé and Butler. But for Butler, "Nabataean" only meant prior to the provincialization by Rome, without any cultural connotation. This is why Jean-Marie Dentzer (Hauran I, 1985) proposed to speak of a pre-provincial period, to avoid any confusion, and today, everyone concurs with this interpretation.

9 All of Sia's documentation shows the Herodians to be the only political force present during the sanctuary's major phases of development, especially between 33 and 1 BCE (see the inscriptions of Maleichathos, cf. now *IGLS* XVI/1, 263 to 264a). Despite a biased reference from PC to Jacqueline Dentzer-Feydy (p. 82, including a cut and paste without quotation marks), the Nabataeans had nothing to do with the installation of the sanctuary. In an effort to date Sia 8, J. Dentzer-Feydy explains that a coin from the beginning of the reign of Rabbel II was found in the foundations, and she concludes that the decoration, probably the work of the same one sculptor found in Sia 8 and the so-called "Nabataean door" in the main sanctuary, therefore dates from the reign of Agrippa II, not Rabbel II as claimed by PC. Sia never ceased to be in the Herodian State since the time of Herod the Great, although there were some interruptions that PC blithely overlooks. Sia therefore has nothing to do with this description of Nabataean culture. The inscriptions there are mainly Aramaic (and not Nabatean), which PC is aware of (cf. p. 87, n. 5), though he does not elaborate on any potential consequences of this.

10 PC readily uses quotations without quotation marks, in a biased manner when a detailed study would be both essential and welcome for a comprehensive and nuanced understanding of their implications, rather than the use he makes, which is both pointed and deformed. For example, he refers to Laurent Tholbecq, 2007 (p. 87, n. 4) to justify his assertion that "after the battle of Canatha, which took place shortly after Actium, the Nabataeans established their domination over this area, even if, approximately ten years later, Herod resumed Auranitis." This, however, is a misguided and fraudulent interpretation, as Tholbecq's article is much more complex: he does not forget that in 31 BCE the region was in Cleopatra's hands and knows that Herod did not "take back," but "received" from Augustus the territories that he had entrusted to Zenodorus, son of Lysanias, after the death of the Lagid queen. The Nabataeans may have briefly occupied Canatha's region, but they were forced to quickly withdraw to avoid succumbing to Rome's wrath. Clearly, a precise knowledge of the political history of the region is essential. PC would have been better off making an inventory of the shrines or dedications dated by the kings of Petra in the Nabataean Hauran, that is to say in the southern part. One wonders if PC choose Sia because the sanctuary is close to Canatha, a city of the Decapolis? He provides (p. 60) the absurd definition cited above of the Nabataean location, which further distorts all subsequent inference and the reader is able to directly observe the result of such problematic interpretation.

11 Chapter 4 ("Decapolis: a Greek island?," p. 88-180) alone comprises half of the book apart from the appendices. PC attempts to define what the Decapolis was, and we can agree with him about the first point: it was a district of the province of Syria entrusted to a prefect (inscription of Madytos), like post-Herodian Judea, before it became a full-

fledged province. He is right to see, as a link between the participating cities, that they were *poleis*, be they more recent or older. But my agreement stops there, as PC does not seem to have understood that this administrative structure rests upon a double foundation: the members had to be a *polis* and part of the province of Syria, but without a geographical contact with the rest of the province. This is perhaps the reason for the absence of Damascus in the old lists, since, in some periods, its *chora* (civic territory) bordered the rest of the province on its western and northern flanks (Sidon and/or Berytos). On the other hand, these cities were separated from each other, for the most part, unless one accepts that they possessed great territories. PC does not even raise the question; he might have known that Canatha (which has nothing to do with biblical Qenath; p. 98) was quite extensive in the West (see my article in *Syria* 58, 1981, which he ignores), but it does not appear to be the case in other directions. For Gerasa, Jacques Seigne has published a number of articles concerning border-marks, which PC does not appear to know and does not reference. Between Canatha and Dion lie villages of the Nabataean kingdom. Adraha was surely not part of the "Decapolis" district in the 1st century as it was a member town of the Nabataean kingdom and did not become a *polis* until the second half of the 2nd century, when the Decapolis had already disappeared. The Decapolis was therefore assuredly not a "Greek island." At most it may be understood as an "archipelago" comprised of more or less isolated islets. To end with the meaning of the word, PC is right to think that the eparchy of the imperial cult created at the beginning of the reign of Hadrian under the name of Koile Syria largely overlaps with the ancient Decapolis, even if some cities were added, like Damascus (which doubtless did not want to pass for "Phoenician" and share the honors with Tyre).

12 Koile Syria: PC encountered this term, which has inspired a great many questions, but he settles the debate in a mere few lines (p. 95-96): it would be the "hollow" of the Beqa at the start, the meaning of which would then have widened. Enlarged as far as Antioch and the Euphrates? This is obviously a mistake, as only Strabo adopts this very narrow meaning, when Theophrastus at the end of the 4th century BCE placed Judaea in Koile Syria, and various texts thus designated the territories between Phoenicia (which is not Syria) and Palmyra or the Euphrates. I think this appraisal should suffice and I will not belabor the point further (cf. *Syria* 95, 2018, p. 447-460).

13 Most of the chapter is made up of a series of exposés devoted to each city member (we know that the lists vary) of the Decapolis. Each review is constructed according to the same model: an introduction referring to the historical texts mentioning the site, an analysis of the urban landscape, completed by the main travelers to the site. All in all, it is a dizzying hodgepodge of information, and one hardly understands what practical use one might make of it. Because PC does not focus on bringing together what relates to the period studied, he collects everything, which inspires an astonishment in him that in turn amazes his reader. For instance, he seems dubious when Eusebius qualifies Canatha as a city of Arabia, close to Bostra; Eusebius is right, naturally, as Canatha was a city of the Roman province of Arabia since the beginning of the 3rd century and Eusebius wrote in the 4th century, when the bishop of Canatha was a suffragan bishop to the Bostra one. As we do not really know what the author wants to demonstrate, we do not know what guided his choice of monuments and texts. PC describes Severian renovations of older buildings, the prosperity of the 4th, and even the 6th century, and goes back to the installations of the Bronze Age: what the connection is with his subject, we may never know. I will not go over the review of each record in detail here,

an entire issue of *Syria* would not suffice for such an undertaking. I will content myself with signaling several errors, which are illustrative of the lack of seriousness of this work.

14 The initial considerations of the chapter underscore how little attention PC pays to the texts. He quotes (with errors) Mark VII, 31, saying that Jesus, going from Tyre to Tiberias, traversed the Decapolis. Anyone familiar with this geography may be surprised: all of the cities of Decapolis are beyond the Jordan River, except Scythopolis, which is quite far South of Tiberias. How is the itinerary given by Mark possible? PC does not ask the question! Jesus actually went back to Sidon first (which PC overlooks). From there, he did not go to Tiberias, but to the shores of Lake Tiberias, according to Mark, which can refer to either shore. If we want Mark's text to make sense, Jesus had to cross Mount Lebanon somewhere North of Tyre (for instance towards Nabatiyeh), then travel towards the South across the Beqa to the Western Jawlan where he could reach the lake. Going down the West bank, there was no chance of skirting the boundaries of any city of Decapolis; but, to the East of the lake, he could pass through Hippos, a city of the Decapolis. Of course, he could have just as readily traveled further to the East before returning to the lake. In condensing his writing, Mark has rendered it obscure and that PC does not raise even a question in the face of such an intriguing text is surprising.

15 In regard to Canatha, PC strictly follows the works of German scholars, never questioning their chronology of buildings, which has nevertheless been seriously contested (cf. M. Gawlikowski in *Topoi* 8, 1998, p. 381-388, ignored by PC). He attributes to me a whim of Klaus Freyberger, referring to an article by Yannis Augier and myself (*Topoi* 2002) as if we were defending the idea of a temple of the "god Rabbou" in Canatha. In fact, Robert Donceel and I (*Electrum* 1, 1997, ignored by PC) had previously shown that the inscriptions clearly attributed the peripteral temple of Canatha to the god Theandrios, "god of Rabbos," according to a formulation very well attested to in the Hauran. K. Freyberger, under some influence, decided that we have to translate *theos Rabbou* not by "god of Rabbos" as the Greek dictates, but by "god Rabbou." This hypothesis, rejected more than a century ago by Charles Clermont-Ganneau, has not been accepted by anyone, except members of his team and PC. To see that PC attributes a hypothesis to me that I have rejected for fifteen or twenty years is righteous grounds for annoyance.

16 For the rest, it has been said that Adraha (the spelling Adraa is far preferable) had no place here, and to be surprised that Pliny did not mention it in the cities is absurd: in his time, Adraa was a town within the Nabataean kingdom. Furthermore, the city of Dion is absolutely located on the tell al-Ashari, on the edge of the Yarmuk and there is no reason to cast doubt on this. It is not known, however, whether the Greek inscriptions dated to the Pompeian era of Dion found in Tafas and Mzeirib originate solely from a looting of the site or if these villages were located within its territory. A simple reading of the introduction to *IGLS* XIV, Beirut, 2016, would have enlightened PC, but he does not seem aware of its existence.

17 In Hippos (p. 117), a building is said to be a *kalybe*, a sanctuary of the imperial temple according to Arthur Segal, whose categorization is followed by PC! PC ignores that this term, used erroneously and throughout Segal's work actually designates very few buildings. A study of this phenomenon was masterfully undertaken by Pascale Clauss-Balty (*Syria* 85, 2008) and is absent from this work's bibliography. Though one may be at

liberty to disagree with her conclusions and one may of course defend a difference of opinion or interpretation, a doctoral thesis must, at the very least, take all relevant works into account by citing them. (The reader should note that these monuments date to the 3rd century CE, and are thus off topic).

18 Why would Capitolias have been founded on land taken from the three nearby cities (p. 127)? Would these cities have revolted in response to such a rare form of punishment? PC gives no argument, provides no textual support for his claims, but this consideration may shed light on something left unsaid: PC believes that the civic territories in the Decapolis are all contiguous. Sometimes, this may be true, but it is false more often than not. Besides, why would this new city reward some Nabataeans, 20 years after the Jewish war (p. 127)? A mystery. Why is the 6th century CE basilica of Abila treated as part of the subject (p. 134)? Is it useful, in this context, to recount the Hellenistic history of Scythopolis (p. 135-139) or the Early Bronze findings of Capitolias (p. 128)?

19 Conversely, the paintings in the necropolis of Abila, which are so instructive for the local culture, are dealt with summarily, in a mere few lines, on the pretext that they are mostly from the 3rd century and are therefore off topic (p. 134-135)! To have the only conclusion drawn be that they bear witness to the owners' "wealthy living standard" suggests that he simply looked at the illustrations (p. 135). And I pass on the prosperity of Pella in the 6th century (p. 147) and so many other instances of "information" poured out without any classification, critical thought, verification or concern for the relation or relevance to the subject at hand. Though it is also true that at this point in PC's text, this perspective is one that has long since been lost.

20 From this jumble of which almost nothing relates to the period supposedly under study, PC nevertheless draws several conclusions at the end of the chapter. First, he tries to define the nature of the Decapolis. As everywhere else, the author possesses neither a clear awareness of the legal procedures nor of the political and administrative history of the region. Contrary to what he seems to believe, a city does not become a *polis* (a "Greek city") because the inhabitants decide it should be so. Since the beginning of the Hellenistic period, a *polis* is always created at the urging of a king, whether the decision is a personal one (as in the case of Antioch, for instance), or at the behest of his constituents (as in the case of Jerusalem). PC does not grasp the link between the cities —their geographical isolation from the rest of the province—and so, understands neither the variability of the lists (a city given by Rome to Herod, for example, necessarily leaves the Decapolis), nor the function of the structure (the Decapolis is not a league of cities; there is no federal-like institution, each city is governed according to its own laws, and the *prôtoi* are simply the principal magistrates of each city). Thus, the Decapolis is merely an administrative subdivision of the province of Syria. The fact that Madytos' inscription dates from the end of the 1st century CE does not mean that the district only existed at that time; most scholars agree that it can be traced back to the time of Augustus, when these cities—despite being included in the province of Syria—were geographically separated from it by client states. Above all, the structure disappeared after all of its cities were split up between the Roman provinces of Syria, Arabia and Judea. The existence of the district only made sense because of the surrounding presence of client states that these *poleis* did not want to be integrated in to and Rome's concern to maintain them within its province. The last client-state in the region disappears in 106.

21 PC's second conclusion is that the construction of spectacle buildings drastically increased in the cities of the Decapolis during Pompey's reign. I am surprised that the exposés in chapter 4 did not pay specific attention to these buildings, and do not discuss their chronology (the monuments are poorly dated). For instance, the theater in Canatha has a Greek inscription, but possesses no date, and it is very dubious that the building existed before the 2nd century CE. We have known for a long time that being Greek was a cultural phenomenon, and the importance of spectacle monuments in all cities, including the oldest ones, bears witness to this: the cities of the Decapolis were endowed with visible signs of their Hellenism. I would not be surprised if they had also built gymnasiums (cf. the very recent discovery of a *sphairisterion* in Canatha, *IGLS* XVI/1). When they achieved *poleis* status, Bostra and Adraa did the same, as well as Philippopolis a little later. Finally, PC devotes a few pages to the topic, though briefly and without the depth of discussion it deserves: the conclusion appears as if from nowhere!

22 In his brief conclusion, PC returns to his primary concerns of globalization, interconnectivity and ethnicity, which are never discussed in any of the pages prior, excepting the introduction. The Mediterranean and Middle Eastern world was unquestionably an open one in which everything circulated, and it is obviously not PC's "study" of a small region of the Southern Levant that will teach us that. But we are pleased to read, in the end, that the cultures of the region were blending (cf. the subtitle of the book), even if, in our opinion and appraisal of this work we cannot say that anything specific to this study has been achieved or revealed that would uphold this affirmation. PC quickly returns to his constant propensity for simplification or not understanding that there were legal rules, political power struggles, even among the Ancients. No, the Nabataeans did not suddenly decide to leave the northern part of their kingdom to the Romans (p. 184). The occupation of the Damascene by Aretas III ended with the arrival of Tigranes in 72 BCE, and after the departure of Tigranes to defend Armenia, a Seleucid king reigned in Damascus, before being liquidated. Who knows who then controls the north of Hauran between 69 and 65: Ptolemy, son of Mennaios, is not improbable, since the kingdom of his son Lysanias extended a few years later up to Canatha. The king of Petra had a sense of political realities, and he returned to his historical kingdom, controlling southern Hauran with Bostra. Obviously, PC ignores everything about the political situation of North Hauran, in particular about the gift to Cleopatra of the Lysanias' possessions between 36 and 31 (see the obscure allusion to an "Egyptian rule" in Sia p. 80, whereas PC affirms that Sia was a part of the Nabataean kingdom). We are also surprised to read (p. 185-186) that after 106 and the rebellion of Bar Kokhba, the importance of the Decapolis increased. Not only do I not understand what the revolt of 130-135 is doing here, as it in no way affected the regions studied, but it is clear that the Decapolis no longer existed after 106! What remained is only the link between these cities for imperial worship within the eparchy of Phoenicia and Koile-Syria. Its separation into two distinct eparchies towards the end of Trajan's reign or the beginning of Hadrian's reign justifies that these cities, on their coinage, proudly displayed their status of "Greek city" or "city of Koile-Syria." And while PC may wonder at the omission, they never mentioned the Decapolis, since it, quite simply, no longer existed at that time!

23 Understandably, my greatest concern is that the publication of a work of this quality, doubtfully meriting the title of doctoral thesis, let alone publication by a well-

established scientific editor, introduces into the field of research far more inexactitudes, and misrepresentations than it does discoveries and new interpretations. Indeed, in my reading I was struck by a total lack of new ideas introduced by the author. I do not know how the defense committee reacted to his thesis, but I am surprised that such a thesis was allowed to be defended without any question and without any visibly applied scientific method; I do not know if the serious errors contained within were pointed out to him and if he simply refused to take them into account. I do not know what validation process was carried out at the publisher that led to the publication of this book. What is apparent, however, is that as an English language publication, this work, as lacking and misinformed as it is, risks becoming an authoritative resource and reference for Anglophone readers. At least I would have warned against this unnecessary and dangerous publication.